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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Typical meteorological conditions associated with extreme nitrogen dioxide (NO_2) pollution events over Scandinavia" *by* Manu Anna Thomas and Abhay Devasthale

Anonymous Referee #1

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In this manuscript, Thomas and Devasthale present an analysis of the meteorological conditions associated with high NO2 concentrations over Scandinavia (as inferred by satellite NO2 columns from the OMI instrument). They find that the highest NO2 in each season is predominantly associated with south-westerly winds, and identify a clear 'transport pathway' using ERA reanalysis. The meteorological conditions tended to persist for either 1 day or 3-5 days, the latter potentially allowing for the longest range transport.

General Comments:

Overall, the paper is well-written with good consideration of related work. The methods are mostly well laid out, and the results are presented clearly. However, I have a couple

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Discussion paper

important concerns.

My main concern is that it is not clear what value has been added from this analysis. As the authors state, the dominance of south-westerly winds during "extreme" pollution is consistent with pollutant transport discussed elsewhere. What has been learned from this particular analysis of satellite NO2 columns in combination with ERA reanalysis data? What new concepts or ideas have been presented? In my opinion, in its current form the scientific significance of this paper is bordering on poor. For publication in ACP, the authors must make a stronger case for the novelty of their findings.

My secondary concern is that they tend to analyze the meteorological conditions during the "extreme" conditions only, without showing us that these conditions are unique to the "extremes" (and thus a defining factor in extreme pollutant concentrations for the region). In other words, the authors haven't clearly shown that the patterns associated with "extreme" NO2 are any different from patterns under more typical pollutant concentrations. What, then, is gained? The "clear transport pathway" could still exist under non-extreme conditions, in which case an explanation for extreme concentrations would need to look at other factors. I suggest the authors prove that these meteorological conditions tend to be specific to extreme NO2 by explicitly showing that the meteorological conditions for the other 90 percent of the time look different.

A more semantic concern: I am not a fan of their use of the term "extreme" throughout the manuscript. Given their definition of an "extreme" event (90th percentile concentrations in each individual season), an "extreme" event in July is nothing like an "extreme" event in January. The analysis is therefore a bit confusing in terms of potential relevance or impact. I wish they would use something a little more qualified throughout their manuscript. For example, could they say "the highest summertime concentrations" instead of "extreme NO2 concentrations".

Specific Comments:

Lines 39-41: Why not include soils amongst the other sources of NOx?

Line 43: One rarely hears NO2 referred to as a strong oxidizing agent with respect to atmospheric chemistry. I suggest rephrasing.

Line 52: I suggest the authors include a more recent projection, given the Lamarque et al. 2005 result is based on IPCC SRES A2 scenario which has very different NOx emission projects to 2100 from, say, the more recent RCP scenarios.

Line 100: Please provide a link to the direct source of the OMI data.

Line 102: "Under cloudy conditions": Please clarify that this means you have not applied any cloud fraction threshold for quality assurance. More importantly, I understand the motivation for this was to include more data (and avoid bias), but these observations result in very high uncertainty. Zien et al. (2014) go through quite a bit of detail explaining their unique treatment of cloudy data, and propose a new computation for the air mass factor calculation in these cases. However, if the authors of the present study are simply using the standard product retrieval (OMNO2d V.3), I expect the cloudy observations to have little realistic meaning. In the best case, over polluted regions with very low cloud fraction, the NO2 tropospheric retrieval still has between 35-60% error (Boersma et al. 2004). This will be much higher for cloudy data. While the authors have made a good case for including the observations, they have not discussed how the poor quality of such observations could impact their results. I.e., what good is a lot of data, if most of it is bad? This must be addressed.

Line 110: Please provide a link to the direct source of the AIRS data.

Line 113: Please provide a link to the direct source of the ERA reanalysis data.

Line 130: Please explicitly state here how the seasons were defined (DJF, MAM, JJA, SON).

Line 137: "It is interesting to see that...": I would argue that this is not at all interesting, but an obvious outcome of their design, given the definition of "extreme" and the seasonality of NO2.

Line 141: The authors note a "bimodal peak" in the distribution of NO2 extreme events. However, the definition of the seasons (DJF, MAM, JJA, SON) is extremely important here. As they have previously stated, NO2 has strong seasonality, peaking in the winter. So, if "spring" is defined as March-April-May, it is not surprising that most of the 90th percentile values for NO2 in this season will occur in March (the closest winter month). Likewise, if the fall is defined as Sept-Oct-Nov, it is obvious that most of the 90th percentile values for NO2 in this season would occur in November (the closest winter month). Thus, the "bimodal" shape of Figure 2b is almost certainly an artifact of their experimental design. I therefore don't understand what we learn from this figure, and suggest removing it, and any discussion of it.

Line 183: Are the specific humidity anomalies calculated with respect to a long-term mean? Or with respect to the other 90% of the NO2 concentration days? Please define how the "anomaly" has been calculated (this would be most appropriate in the Methods section).

Line 203: "higher wind speeds.. during extreme events compared to climatological conditions": Where are the climatological conditions shown? As I mention above, I suggest including figures of the climatological meteorology in all cases, for a more obvious comparison to the "extreme" conditions.

Line 209: If I wanted to reproduce this data, how was "persistency" actually determined? I.e. what computational steps were performed on the meteorological data to evaluate this.

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